Patterns of innovation

Part I

F R Bradbury, M C McCarthy and C W Suckling

The importance of models of innovation

Industrial research scientists are aware of the significance of theories which are used to explain chemical and physical reactions, and often discuss these theories quite explicitly. Much less frequent, however, is discussion of any theory or model of industrial innovation. Yet innovation is the objective pursued by industrial research scientists, and the implications of a theory adopted to describe innovation may be as considerable as, if not greater than, those associated with scientific theories. It may therefore be fairly claimed that to discuss theories or models of innovation is not to indulge in a purely theoretical activity; it is also to discuss a topic of great practical importance. This should not be surprising: the making explicit of the implicit assumptions guiding men's actions often displays the power of these assumptions. The actions of practical men of affairs, as Keynes so famously remarked, may be ruled by the theories of defunct economists; the activities deemed worthy of pursuit by academic scientists, as Kuhn has argued, are defined by paradigms infrequently challenged.[1] So too, the way in which practising research scientists perform their job is profoundly affected by the theory of innovation in which they believe. Its power is the greater on account of the infrequency with which it is explicitly considered.

In this article, we wish to examine two models of innovation in some detail, and to consider the significance of each for the practising research scientist. In two subsequent articles, the model adopted will be used to examine two major innovations within the chemical industry, both originating with inventions made within Imperial Chemical Industries.

Definition of innovation

Before discussing models of innovation, it is important to reach agreement on the nature of the beast itself. Innovation is here treated as a process, which originates with the recognition that an opportunity or a threat exists, and which is concluded when a practicable solution to the problem posed by the threat has been adopted, or a practical means of grasping the opportunity has been realised. Innovation must be distinguished from discovery, by which we mean the finding or uncovering of new knowledge; and from invention, the discovery which is perceived to possess utility. Both these processes may, but need not, occur within the larger process of innovation. The treatment here defined suggests several questions to be investigated: what is the mechanism by which opportunities are seen? Why do some firms – or individuals – see opportunities or threats which do not impinge on the consciousness of others? What may be involved in the innovative process, either in the form of organisations, or of incidents? What are the incentives that encourage those who embark on the innovative process, and what are the barriers that they must overcome?

As well as considering these questions, it is necessary to decide what characteristics of innovation must be explained by an acceptable model. We suggest that three characteristics must be accounted for. First, and most striking, only a very small proportion of research ideas will survive to become innovations. This is indicated for instance by the fact that in Britain, only 2 per cent of patents survive their full life of 16 years; in the pharmaceutical industry 3000 compounds must be synthesised for each one proved in clinical trials; American experience suggests comparable mortality curves.[2] The second feature of the process from research to innovation is the extended time scale: an arithmetic mean of the time elapsing between invention and subsequent innovation for 35 innovations examined was 11 years; examination of weapons research showed that the average time between two generations of weapon systems was 13 years.[3] Third, a feature of the progression from research idea to innovation is the frequency with which a research target may change. A dramatic example is afforded by a study made of six fighter-plane developments, which showed that four ended with different engines, three with different electronic systems, and five with extensively modified airframes from those originally envisaged.[4] Other, less dramatic, examples abound: the insecticide 'Gammexane' was first considered (and rejected) as an insect repellant rather than an insect killer; the colouring material phthalocyanine first found use as a pigment, though its inventors believed it to be a dyestuff; the bipyridyl herbicides were shown to possess properties very different from those first sought.

In short, it may be claimed that the population of research ideas is characterised by high mortality, slow maturation, and adaptation with some outright mutation. On its ability to account for these characteristics and to answer the questions indicated above must any model of innovation be judged.

F R Bradbury is professor of industrial science, Stirling University; M C McCarthy is a member of the Business Group, Petrochemical and Polymer Laboratory, ICI Ltd; and C W Suckling is deputy chairman of Mond Division, ICI Ltd

The 'invention-based' model

Probably the most widespread model of innovation is that which emphasises invention. Many practising research scientists define their objective in terms of invention, and their actions may be explained by this model. This type of model is associated with the influential book *The sources of invention*, by Jewkes, Sawers and Stillerman. Their thesis may be simply represented as saying that invention is a separable and isolatable activity, which may be studied without reference to subsequent development. The movement from invention to the invention's eventual fulfilment as an innovation may by represented as a linear progression, on which path the invention represents the starting point, an identifiable and separate step. This step, it is argued, may be taken just as easily by small as by large firms; it is the result of determined and original men; and is constrained by the decision-taking processes of large corporations.[5]

The influence of this model has been widespread. The assumptions that innovation is a linear process traceable back to some identifiable origin, and that this origin is worthy of study by itself, may be found in many discussions of innovation. And – probably as a consequence – the assumptions are adopted by many research scientists that success may be equated with the creation of new knowledge; that their main task is discovery; and that chance event, unconstrained by too rigorous knowledge of development problems, is the way in which large innovations originate. The examples put forward by those who adopt these assumptions include the invention both of polyethylene and of the pigment and dyestuff phthalocyanine. Both bear examination in more detail.

The influence of the theory associated with Jewkes and his co-authors has been so great that the theory itself merits closer examination. In particular, two questions may be raised: is it true that in studying invention one is studying the causes of innovation? There is much evidence that suggests that invention may occur without subsequent innovation – the most likely future for any new idea is that it will be consigned to oblivion, or enshrined in the literature, without producing innovation – and also that innovation may occur without an invention. Some industries – the textile and agricultural industries, for example – show considerable growth and progressiveness without displaying inventiveness. The speed with which a country used synthetic materials is related not to its ability to discover new polymers, but rather to its ability to imitate what has been invented elsewhere.[6]

The criticisms that have been raised against the 'invention-based' model of innovation may be summarised by saying that the model fails to recognise particular features of innovation. Since it fails to recognise these characteristics it provides little help for the research scientist who is grappling with particular problems, other than the encouragement that many successful inventors had to struggle long and hard to succeed. And, even were the first question to be answered in the affirmative, a second question is whether invention can meaningfully be discussed in isolation from development and subsequent stages in the innovative process. Again, it is doubtful whether this is true: analyses of research projects suggest that there is no simple proportion of research ideas that progress to development. Rather the type of research, the definition of the research target and the relationship between research and other departments in an organisation influence the likelihood of success. Yet all these are ignored by a model of innovation that concentrates on invention as a separate phenomenon.

Barriers to innovation

A second, and less well-known, model of innovation concentrates on screening new ideas. This has been discussed by Baines, Bradbury and Suckling, who draw an analogy between research activities and the well studied problem of a search strategy. They identify the start of the innovative process as the time when the perceived qualities of a new material and the perception of a need are brought together. The bringing together of these two patterns of properties, and their modification, constitutes the invention, and the subsequent survival of the material whose potential usefulness has been recognised comprises the innovation. Baines and his co-authors discuss this process as the progression of a new material through a series of screens, and discuss the properties and pathology of screens. By a screen, they mean a deliberately erected barrier, whose mesh is so designed to allow those projects to proceed whose profile of properties, or a selected few properties, match those that have been identified as important.[7]

A model which emphasises the screening mechanism by which research projects are judged concentrates attention on the rejection mechanism involved in the innovative process. This provides some explanation for the number of research ideas that fail, and for the way in which modifications occur. It is useful to extend the analysis, by elaborating the concept of screens. This may be done in two ways. The first is to consider the progression of an invention, by identifying the number of organisations which may be involved between the original perception of need, and the need's being successfully satisfied. The implications of this may be most easily examined by considering the places where the original perception of a need may be made. Simple examples include recognition of a need or opportunity by:

1 the end user (the farmer who uses existing polythene sheet to retain moisture and kill weeds)

2 the manufacturer, or potential manufacturer, of a product (the manufacturer of polythene sheet who persuades the farmer to use it)

3 equipment manufacturers (the manufacturer of machinery to enable polythene film to be used for shrink wrapping)

4 manufacturers of intermediates (those responsible for introducing nylon and terylene into the textile industry)

5 raw materials suppliers (the National Coal Board developing new central heating boilers)

6 suppliers of ideas (the inventor of cats' eyes, or research associations).

This simple enumeration of possible points at which the original perception of need or opportunity may be made is illuminating, since it may be seen that in only one case may the innovation be implemented without involving more than one organisation. In all instances of product innovation other than those when the need is recognised by the end user the success of the innovation depends on persuading other organisations to participate in the innovative process. Example (5) above illustrates the point: to sell more coal, the National Coal Board has to develop, with equipment suppliers, new boilers; their success depends on several other organisations' cooperation. This may be represented by a simple diagram, which illustrates the potential interaction between organisations necessary to achieve an innovation, as distinct from an invention. This figure shows that it is likely that between the recognition of need and the fulfilment of it, various organisations, each with its own objectives and criteria, must be involved. Each may possess its own objections to a new idea, and each may be motivated by different aims.

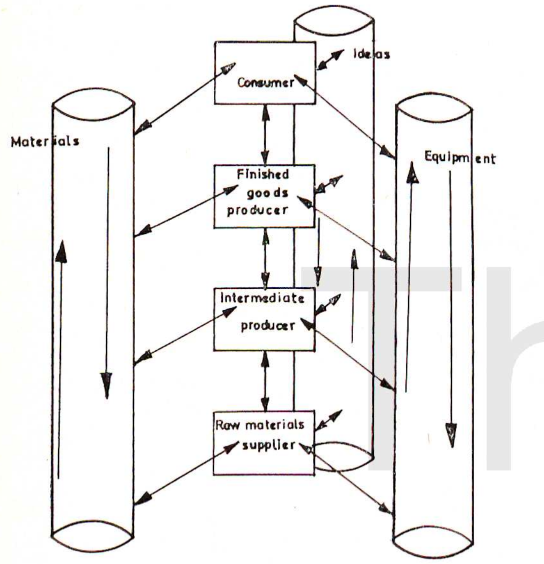

Fig Conceptual model of innovation

The conceptual model of innovation represented in the Figure has the following features:

1 It is a process, in which a new technique, product or manufacturing process may be translated from an idea to a complete achievement

2 The process is marked by a high rejection rate, and by some change in the direction and nature of emerging projects

3 The process is lengthy. It shares characteristics with many other processes of problem identification and problem solving encountered by industrial managers: a difference is perceived between a target and the level presently achieved; solutions to this are searched after, by doing research, and are tested. This cycle continues until the target has been achieved. The process differs from many managerial cycles of decision-making in that it is unlikely to involve just one firm, and still less one department. To complete an innovation, the cooperation is likely to be required of several firms and organisations, each with its own objectives and constraints. The overall cycle of problem recognition, and of search for, testing and implementation of solutions is correspondingly more complex

4 The innovative process may therefore be considered as technological development within one organisation, followed by technology transfer to another organisation, which may then have its own process of technology development. A detailed mapping of the process of technology development in the chemical firm has been suggested elsewhere[8]

5 Technology transfer, which in its later stages may be identified with diffusion, may carry an innovation through a number of firms or industries. The adoption of a technology by firms within an industry may mark the end of the diffusion: this may occur when the innovation comprises a new process for making an existing product. Alternatively, it may give rise to new innovations later transferred to other industries: thus new synthetic fibres led to new fabrics

6 The researcher in the chemical industry, although he will benefit from useful and essential feedback from technology users, to succeed must often be extremely active in encouraging and effecting technology transfer to other industries. In the Figure, the chemical industry may be most readily identified with the intermediate producer.

The significance of the model as now elaborated for the research scientist is considerable. His task assumes new dimensions. No longer may he remain devoted to discovery and the creation of new knowledge, for the model emphasises that invention represents only one part of the innovative process. Rather the research scientist's tasks include – if innovation as distinct from invention is his aim – the identification of the other protagonists in the innovative process, and the elucidation of their aims. At the same time, invention itself may be treated as a less random and less inexplicable event, for the emphasis lies on the bringing together of new materials with the awareness of needs. The model suggests that as important a precondition for invention as the creation of new phenomena is the awareness of need. For without the awareness of need, the new phenomena may never make the industrially all-important transition from being – as is all new knowledge – interesting, to being industrially significant: or may never move from a discovery to being an invention.

The example of polyethylene points the problem. The original discovery of a waxy compound subsequently identified as polyethylene was made at Winnington in March 1933, yet it was not until 1938 that Habgood suggested that it might be used as a gutta-percha substitute, thus bringing together material and need.[9] Today, the amount of polyethylene used in electrical applications is small in comparison with that used in other applications. The original linking of science and need which led to the gutta-percha substitute has been repeated by the identification of many uses of the bulk polymer; each recognition has required a new innovative chain to be constructed to achieve an innovation. The example of polyethylene can be used at least as well to stress the importance of 'use-recognition' as it can to stress the importance of un-targeted research.

The first elaboration of Baines, Bradbury and Suckling's treatment of innovation has therefore been to stress the need to construct an innovative chain between discovery and innovation. This has emphasised the need to identify the protagonists involved, and the various criteria they adopt. It has also shown the importance of 'use-recognition', the moment at which a discovery becomes an invention.

The second elaboration of the model is the structuring of the barriers to innovation within the innovative chain. Here it is useful to consider problems under different headings.

Economic barriers

Various economic barriers to innovation may be identified. First, the organisation undertaking research will only do so if it believes this to be profitable (although not necessarily in the short run). Secondly, the purchaser of a new process or product must be convinced either that he is gaining the same end results as before at lower cost (*eg* a new process to produce an existing product at reduced cost) or that the extra properties which he acquires with the innovative product sufficiently compensate for any extra cost. Thirdly, it is likely that some organisation may have to invest in capital equipment to use the research results.

Technological barriers

Two types of technological barriers may be identified. First, a scientific or technological target must be achieved: a chemical yield increased to a certain percentage, or an electronic component reduced to a certain size. This particular barrier is one well-recognised by research scientists, and one to which they devote a considerable proportion of their efforts. It is perhaps not surprising, therefore, to find that analysis of the causes of failure in research laboratories has shown that failure because of unforseen technical difficulties is not the main cause of failure: one analysis showed technical problems to be a less important cause of project failure than managerial decisions on manpower allocation; others have shown that about 30 per cent of all failures, or cost and time overruns, for research on defined targets is attributable to technical problems.[10]

The second type of technological barrier is one which receives far less attention. This is the barrier related to the probability that an invention made in one organisation will be implemented by, or affect, other organisations; and must therefore be compatible with, or alter, existing techniques and processes. The experience is common: new equipment for detecting aircraft required new communication systems; new dyestuffs, new dyeing techniques; new electronic equipment new standards of cleanliness among material suppliers. The introduction of a new herbicide may require new equipment and techniques to be used by the farmer; the advent of a new anaesthetic may require anaesthetists to change their ways. These changes are not optional: for the new product or process to be used, they must be made. The price of the new product may include considerable investment in new processes to use it. Despite the importance of this barrier, there is not much evidence of attention being paid to it, and of effort being devoted to predict it; the model of innovation here discussed emphasises its importance.

Organisational barriers

There is much discussion of organisational barriers to innovation, mainly centred round the Not Invented Here (NIH) factor. The assumption is often made that this reaction is irrational, the expected response of man in an eighteenth century environment dominated by knaves and fools. And once such an assumption has been publicised, it may well act as a self-fulfilling prophecy: the publicised assumption that someone is a knave or a fool is likely to alienate.

How far is this view justified? The model of innovation here discussed suggests a more rational explanation of the NIH factor: different organisations, and different parts of the same organisation, may have different targets and criteria, and prior commitments. Since motives may diverge, and since the language used to express even closely similar targets will differ, the innovator must be aware of many requirements on his invention, and must be able to use the appropriate method to communicate its advantages. NIH, we may suggest, can represent not only an emotional Not Invented Here, but also an arguably logical Not Interested Here. And the latter response will be overcome only by demonstrating the potential or actual usefulness of an invention to each member of the innovative chain.

Personal barriers

Organisational barriers are often compounded by the existence of personal barriers, arising from the different backgrounds and targets of different groups. Some studies have suggested conflict between the objectives of scientist and of manager, and communication problems between the two groups; others that research divided on functional lines – into, for example, a physics group and a chemistry group – has significantly greater communication problems than that organised as project teams. Again, this sort of conflict, and this barrier, is easily reconciled with the model discussed here.

Perceptual barriers

Closely allied to both organisational and personal barriers are perceptual barriers. These perhaps more than any other type of barrier are neglected by research scientists, and yet can profoundly influence the likelihood of an innovation's success. Two particular categories of perceptual barriers may be identified. First, different groups in the innovative chain may possess different criteria against which to judge new ideas. An account of the difficulties encountered by an economist and an engineer when discussing a simple, but extremely effective, water pump illustrates this. The engineer was concerned with engineering complexity, the technical skill required to manufacture the pump, and its mechanical efficiency; while the economist judged the pump by comparing the costs of pump rental against labour charges for traditional means of raising water. To the former, the device was crude and uninteresting; to the latter – and to the peasant wanting to raise water – simple and useful.[11] Another situation, not infrequently encountered, occurs when the scientist or engineer has a new product of considerable scientific complexity, which is difficult to describe in terms attractive to the customer.

Second, the new product is often judged against criteria which have been specially designed for an existing product. This may well present difficulties. Rayon, for example, was introduced as 'synthetic silk' and was rejected by many since it did not approximate more closely to the properties of silk. The example shows that there are circumstances in which it is important that the new product should be seen as an entirely new product, judged on its own terms, rather than as a substitute for an existing product. If the new product is compared with the old, and judged on the terms used to judge the old, comparison may suggest that the new is more expensive or less effective, despite other cost savings or advantages in the system of which the product is but one part. Thus it may be important – and difficult – to persuade customers to change their criteria in order to change their product.

So far, the discussion has suggested that the five barriers identified – economic, technological, organisational, personal and perceptual – are necessarily obstacles to the creation of an innovative pathway. It might be more accurate to claim that each represented a problem area, where barriers are likely to impede successful innovation, and where work to anticipate and overcome barriers by appropriate inclusions in the R and D programme may facilitate innovation. There is an obvious appeal between the concept of a screen within the laboratory or research organisation, and that of a barrier existing outside. Indeed, the purpose of discussing barriers is to predict them in practical circumstances, so that work may be done early in a project's life to test the vulnerability of the project to the harshness of the environment into which it must progress.

There is a danger, however, of failing to stress the positive aspects of the model. For the concept of an innovative chain, supported by the structuring of possible barriers to innovation into five classes, allows the determined research scientist to do more than merely recognise the hazards his project must escape. One question that this analysis brings to his attention is that of motivating the protagonists in the innovative chain: 'why should the engineer, or the wholesaler, or the equipment manufacturer, help realise this idea?' One remark often heard, which we support, is that 'things happen because people make them happen'. As important is 'things happen because people see the advantages of making them happen'. The model here discussed stresses the need first to identify the protagonists in an innovative chain, and then to discover their objectives and constraints. For without detailed knowledge of the protagonists' ambitions and problems any appeal to gain their assistance in furthering the innovation runs the risk of missing its target; and the danger increases that effort will be devoted to research that is not likely to complete the dangerous journey to become an innovation.

We do not argue that it is necessary, or indeed preferable, to start R and D activities in response to a perceived need. The argument is not about where one starts, but about how one proceeds. Innovation may originate in either the perception of a need, or in the recognition of the opportunity presented by a scientific discovery. Such is the power of science that many innovations will continue to occur in the second way. Irrespective of its origins, innovation comprises an extenuated process by which the demands of the market are linked with the strengths of scientific and engineering knowledge *via* a technology. If the model adopted here is accepted, there is a case for trying to see both bridgeheads – the scientific or technical and the perceived needs – together, and it becomes essential to build the technological and organisational structure to connect the two. To focus exclusively on the technical bridgehead is to incur the risk of severe misallocation of effort; to focus on needs may result in potentially relevant scientific knowledge remaining unutilised.

Conclusion

Two models of innovation have been considered. The first is based on invention, which is treated as an isolatable step in a pathway that leads to innovation. The second is concerned with the progressive bringing together of a perceived need, and a compound, material or system with a perceived set of properties, and their modification to meet the needs of those comprising an innovative chain. Inevitably, in the space available, each model has been treated briefly. It is apposite, however, to return to the original remarks at the start of this article, in which we claimed that the theory of innovation was important insofar as it affected the practices of working industrial research scientists (although we believe it also to have relevance in an academic context). What differences in practice may be derived from the two very opposing theories that have been discussed?

The main difference is the way in which the research scientist in industry defines his task. The research scientist who believes in the first model will define his objectives as seeking new discoveries, and is likely to concentrate on the building of new knowledge. In some ways, his work will not be unlike that he has performed at university. The work of the research scientist who adopts the second model will be very different. He will concentrate not solely on the creation of new knowledge, but also on the assessment of the significance of knowledge – either new or already existing. Scientific excellence is still required, but he will recognise that scientific excellence by itself is not sufficient. Whereas the former defines his targets in scientific terms, the latter is more likely to define his objectives in terms of technological achievements. The latter, operating with a wider agenda, is likely to be more aware of the constraints established by the technologies, manufacturing processes and commercial practices which connect the needs and demands of society with the scientific skills of the research scientists. He may consequently be more willing to supplement his skills with non-scientific skills.

The implications for resource allocation within a research laboratory are considerable. For the first model will lead to an emphasis on research workers at the bench, working on the discovery of new knowledge; other expense may be viewed as overheads, to be reduced as far as possible. The second model leads to a greater emphasis on activities other than those of the research worker at the bench. In particular, screening relative to the creation of new knowledge assumes a new importance, and it is recognised that the definition of appropriate screens may in itself be a major research undertaking. Finally, the two models have significance for the actual organisation of research. The former is more likely to result in research groups based on scientific divisions – a physics group, or a chemistry group. The latter suggests that in many instances a more suitable organisation may be one based on end applications – a textile auxiliaries group, for instance.

The second model is, we believe, a more accurate depiction of the innovative process than that based on invention. It is both more helpful to the practising industrial research scientist and more useful in elucidating case studies of innovation. We also believe that it points the way to a more satisfying and socially relevant role for the research worker in industry.

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Dust explosions in factories

B M O'Reilly

A dust explosion is the rapid combustion of particles of dust suspended in air. The heat produced by the combustion of the suspended particles causes a rapid expansion of the surrounding gas which in turn produces an increase in pressure at the walls of the vessel containing the dust cloud. Additionally during combustion gaseous products may be evolved which again cause increased pressure. In factory processes it is the effect of this pressure wave on relatively weak confining structures, for example, dust collecting plant and factory walls, that causes a great deal of concern to people handling materials likely to give rise to this phenomenon.

Nearly all carbonaceous materials if they are in a sufficiently fine state can give rise to a dust explosion. In addition certain metals can also give rise to this phenomenon and generally metal dust explosions are more violent than other dust explosions. A list of dusts that have been subjected to laboratory tests to determine whether or not they are likely to propagate flame in the form of a dust cloud is published by HMSO as SHW 830. The list divides them into two groups:

Group (*a*) – dusts which ignited and propagated flame in the test apparatus

Group (*b*) – dusts which do not propagate flame in the test apparatus.

The above qualitative tests are an arbitrary guide to dust explosibility. Additional tests to determine such parameters as rate of pressure rise, maximum pressure produced by a confined dust explosion, electrical spark energy necessary to ignite a dust cloud, dust cloud concentration necessary to propagate flame, and ignition temperatures, can all be determined and do assist in assessing the explosibility of a dust cloud of a particular material. The examples in Table 1 illustrate the different explosibility of different substances.

Generally speaking the finer the dust the greater its explosibility, *ie* the ease with which it is ignited is enhanced and also the rate at which pressure rises in a confined explosion increases as the particle size decreases and similarly the maximum pressure produced increases. The most explosible dust is the dust that passes through a 200 BSS mesh sieve since this seems to be the limit to which the effect of decrease in particle size affects explosion parameters. The next particle size which has some importance is about 60 BSS mesh for carbonaceous dusts. Aerodynamicists have indicated that coal dust of 60 BSS mesh size can be thrown into suspension by the turbulence ahead of an explosion flame and in this way a propagating dust explosion is formed. It should be noted that this is a fundamental distinction between a gas and dust explosion. In a dust explosion, dust can be thrown into suspension to feed the flame while a gas explosion will not continue beyond the confines of the explosive mixture.

As can be seen from Table 1 dust clouds do exhibit explosion limits. In most cases the lower explosive limit is clearly defined whereas the upper explosive limit is often ill defined. In the latter case this seems to be due to experimental difficulties in producing a dust cloud of the required concentration with exact repeatability and also there are distinctions between what the experimenter considers propagation of flame and non-propagation of flame. Between

Five men were killed and two seriously injured by an explosion of maize husk dust in this factory

B M O'Reilly of HM Factory Inspectorate, Chepstow Place, London WC1 presented this paper at the symposium on 'Safety in the chemical industry' held by the Fine Chemicals Group of the SCI on 17 February 1971